

RESEARCH INSTITUTE FOR MICROBIAL DISEASES
UNIVERSITY OF OSAKA
OSAKA, JAPAN

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Dear Dr. Lederberg:

Sincerely thanks for your kind letter of July 9, which I received the day before yesterday. Having read your letter, I was very pleased to know that you felt some interest in our experimental results achieved on the antigenic variation in *T. gambiense*, the details of which I am sure to write you within about a week.

Here I should like to answer briefly to your question about my mention "fission is not required for the development of serological changes". Of course, I can say with confidence that the studies of ours was the first report being able to display the changes actually occurred in the same treated individuals.

It is well known that the trypanosoma once agglomerated with the antiserum will disperse themselves again in time, if the antiserum properly diluted is used. I supposed that this disperse phenomenon might be caused by the antigenic variation of *Trypanosoma* itself, beginning to occur within a comparatively short time after exposing to the antiserum in vitro. On the other hand, we found a new directed variation system of *Trypanosoma gambiense* in the infected mice, according to which we became to enable to examine for the question whether the agglomeration (not agglutination) would reappeared with the ^{antiserum} antiserum of destined serotype after having dispersed the first formed agglomerates of trypanosoma with the corresponding antiserum. By this method, we could catch all the steps of the hereditary, antigenic changes under the microscope. Therefore, I can say with confidence that I succeeded in demonstrating the antigenic variation in the same treated individuals.

Of the another problem on the blephaloplast in the trypanosoma, I have recently read the Piekarski's report titled with "Blephaloplast und Trypaflavinwirkung bei *Trypanosoma brucei*," referring in Trop. Dis. Bull., Vol. 41, p. 21, 1950, in which I was also much interested. Therefore, I am intend to direct one part of our workings towards this interesting and important problem. Moreover, I am supposing that the blephaloplast may be some related with the antigenic variation in *Trypanosoma gambiense* studied by us. Nevertheless, I am very regrettable to say that we have had no other papers on this kind of project here in Japan.

I had studied on Bacteriology for about 10 years under Dr. Tenji Taniguchi, Professor of Bacteriology, Dean of our Medical School, and Head of our Institute, during which I had worked chiefly in the field of virus, richettsia, and malaria. Consequently, I adopted the immunological, ~~genetical~~ ^{as my major field} and biochemical speres of Protozoology. As the first step, I had launched the studies on the immunological variation and mutation in several protozoas. However, I became more and more to feel keen interest in the genetical aspects of microbiology, which was inevitably important to solve several questions concerning to the immunological variation and other variations in the protozoas. Inspite of this, I am very sorry to say that I have only insufficient knowledge of genetics, therefore I have had an earnest desire to go to your country to study on genetics for a while, if possible.

You described that you have hoped to find a student who would be interested to work the mechanism of formation of aparabasal forms. If you would kindly give me the oppotunity to study on genetics and to work on the protozoa, I should be very much pleased and oblided to you.

I am very much familiar with the name of your university, Wisconsin, because I had read the papers on Trichomonas foetus reported by Dr. late Morgan, former professor of Veterinary Medicine, your University. I also have been studing on both T. foetus and T. vaginalis and have achieved some interesting data from the point of genetical view, of which I shall write you in future. Furthermore, I am very pleased to write here that Dr. Osamu Hayaishi, an assistant of our Institute, a promissing bacterio-chemist in Japan, had been in the Enzyme Institute of your university to studies on the adaptive enzymes in bacteria under Dr. Green. However, in December, 1950, he removed to the Microbiological Institute of National Institute of Health, Bethesda, Md. He had always admired the modernized laboratories there and earnestness of the students working there.

In closing, I should be grateful to your kindness to present me your useful reprints and to place my name in your mailing list.

At present, I have been discussing on the antigenic variation in T. gambiense with Dr. Sonneborn, Indiana University, who wrote to me that my experimental results is wounderful and somewhat different from his data achieved on Paramecium aurelia. I sent the precise description of our studies on T. gambiense.

With best wishes,

Sincerely yours, *Shozo Inoki*